THE GEOLOGY OF THE SAN FRANCISCO PENINSULA.

THE Coast Ranges of California embrace an extensive region in which are presented complicated but exceedingly interesting geologic problems. Much attention has been given to these mountains for many years, but with the exception of a study of the quicksilver deposits, it is only recently that we have had presented in a thorough manner the results of the detailed examination of local areas.

Professor Lawson's recent report upon the geology of the San Francisco Peninsula[1] is perhaps the best yet made of any local area in the Coast Ranges and illustrates well what modern methods of research can accomplish in a complicated field. Notwithstanding the excellencies of the report, there is an infelicity displayed in the discussion of several problems which is regrettable in a study of this kind. This has, however, necessarily resulted from the method pursued, in that the investigator has given his attention almost exclusively to a narrow field of complex geology and has failed to make use of the results of the work of others, concerning questions which the phenomena in that field did not illuminate. It is absolutely necessary for the appreciation of many facts in any local area, and for the philosophic discussion of the history of that area, that the student should have a general knowledge of the relations existing over the region as a whole.

As a result of some experience in the Coast Ranges I feel called upon in the interest of geological progress to express most profound objections to a number of conclusions reached by Professor Lawson concerning some of the vital questions involved in the geological history of this region. Consequently in answer to his frank request for friendly criticism, I will take up the dif-

[1] U. S. Geol. Sur., 15th Annual Report, pp. 405–476.

ferent points which I feel are open to question in the general order in which they occur in the report. There are several statements in a synopsis of this report,[1] published a year and a half ago, which will be included in the criticism. There is a failure, which is without doubt due to an oversight, to give recognition to some contemporaneous and earlier work in the same general field and upon the same topics.[2]

Passing over the Montara granite and the associated marbles which exhibit the same relations and have without doubt the same history as similar rocks in the Gavilan and Santa Lucia ranges, we come to the author's Franciscan series, the oldest uncrystalline terrane. Professor Lawson divides it into five petrographic divisions. The lowest consists of conglomerate, sandstone, shale, etc., and is well exposed at Point San Pedro. He considers that this division may possibly be older and underlie the strata north of San Pedro valley unconformably, because fragments of shale similar to that at the point occur in the sandstone north of the valley. The fact seems not to have been noticed that fragments of similar shale are found in the basal conglomerate on the point. The conglomerates are identical in character with those at the base of the Golden Gate series on the Monterey coast, and are without much doubt of the same age.

The "foraminiferal limestone" and "radiolarian chert" form perhaps the most interesting portions of the series. They are dwelt upon in detail, particularly the "cherts." According to Professor Lawson the latter are hard siliceous rocks of varying degrees of purity, and are "prevailingly of a dull brownish red color, although other shades occur." He says that "in many

[1] Am. Geol., June 1895.

[2] Bull. Geol. Soc. of Am., Vol. XI, pp. 71–102. The results of this work were read before the Geological Society of America nearly a year in advance, and published six months prior to the first of the papers under discussion. The subjects concerned were the position and character of the marbles of the Coast Ranges, and especially the rocks constituting the Golden Gate series (Franciscan series of Professor Lawson), the nature of the sandstones and the radiolarian origin of the jaspers, as well as the geologic position of the series.

cases they are true jaspers and have been so designated in some of the earlier descriptions of them." After discussing their variability he again says: "In view of this variation in petrographic character, it has been deemed best to refer to these rocks by the old and familiar name of 'chert.'" It seems to me that, on the contrary, the designation "chert" is not only not as appropriate for rocks of this character but it does away with "an old and familiar name" for no sufficient reason. The designation "jasper" was used by Blake, Newberry, and Whitney. According to Geikie, "Chert is a name applied to impure calcareous varieties of flint in layers and nodules which are found among the Palæozoic and later formations, especially but not exclusively in limestones."

The siliceous bands in the foraminiferal limestone which are referred to by Professor Lawson as "veins" I do not consider such in any true sense of the word; they are more properly cherts or phthanites according to the original use of the terms, but in regard to the great body of siliceous rocks occurring independently there seems to be no use in making a change in terms.

The nature of the so-called "veins" in the limestone seems not to have been clearly understood. On exposed surfaces of many feet in extent these siliceous bands stand out with great distinctness. Some of them are as even and regular as the limestone strata, while others are discontinuous and more uneven. They sometimes blend into the limestone but more commonly are quite sharply distinguished. These bands of chert are contemporaneous deposits, being always conformable to the stratification of the limestone, and differing most markedly from the veins of secondary origin. Thin slides prepared from a number of specimens show them to be thickly filled with organic remains of radiolarian character.

After a description of the jaspers several theories of their origin are considered. The theories are as follows: (1) siliceous springs in the bottom of the ocean, similar to those well known in volcanic regions; (2) radiolarian and other siliceous remains which may have become entirely disssolved in sea water; (3)

volcanic *ejectamenta* which may have become similarly dissolved. The two latter are rejected and the first received, as having the most to support it, in the following words: "The hypothesis of the derivation of the silica from siliceous springs and its precipitation in the bed of the ocean in local accumulations, in which the radiolarian remains became imbedded as they dropped to the bottom, seems, therefore, the most adequate to explain the facts, and there is nothing adverse to it as far as the writer is aware."

Some time since I proposed a theory[1] to account for the origin of the jaspers substantially equivalent to the second given above. Professor Lawson's chief objection to the view of the organic origin of these rocks consists in the fact that they occur in lenticular masses instead of evenly bedded deposits. In his petrographic description it is stated that the "cavities of the radiolaria have been filled with chalcedonic silica and are in definite contrast with the non-chalcedonic matrix." With this last statement my experience is not often in accord. I have found every gradation in the specimens from those in which the radiolaria are distinctly marked, as Professor Lawson says, to those in which they are only faintly distinguishable from the matrix, or apparently absent. In my opinion this state of things gives good ground for the view that, owing to possible transformations through the action of sea water, and the secondary changes which are known to have taken place, there is no valid reason for denying the organic origin even when no organic remains are distinguishable.

Professor Lawson has failed to recognize that the siliceous bands in the limestone must have had an origin similar to those occurring in aggregates by themselves. If the theory of formation by springs is applicable to one it is to the other. The occurrence of these radiolarian jaspers interstratified with the limestone is a most suggestive fact. Similar conditions of sedimentation must have obtained in the one case as in the other, the only difference being that at one time calcareous layers were

[1] Bull. Geol. Soc. Am., Vol. VI, p. 85.

formed, and at another siliceous. The local bands of silica, a few feet perhaps in lateral extent, wholly surrounded by limestone and sharply differentiated from it could not in all probability have been formed through the action of siliceous springs. The large lens-shaped bodies of massive jasper are also abruptly marked off from the inclosing sandstone or shale. If each lens were due to the action of one or more springs the currents must of necessity have been weak near the edges of the deposit and could not possibly be conceived as "sufficient to deflect sediment-laden counter-currents." If sedimentation were going on around the springs it is impossible that the silica could have been precipitated without more or less commingling with the sand. The presence of similar radiolarian jasper in the limestone is suggestive of the view that to whatever cause the lenticular form of the latter is due the same cause conditioned the similar outline of the former.

Turner[1] says of the limestones in the Knoxville at Mount Diablo, and of the older limestones in the Sierras that "each calcareous layer is rather a series of lenticular bodies than a continuous limestone stratum. This applies to the Sierra Nevadas as well, only there the limestone bodies are hundreds of feet in diameter." The belt of foraminiferal limestone described by Professor Lawson is equally bunchy, reaching a very considerable thickness in places, as on the northern slope of Black Mountain, and then contracting to very limited proportions. It thus appears that as far as the shape of the deposits is concerned the spring theory of origin is equally applicable to the limestone.

The statements in the report concerning the condition of the formation of the jaspers do not exactly accord. For instance, on page 426, Professor Lawson says: "If the springs were strong the currents engendered might in some places have been sufficient to deflect sediment-laden counter-currents, and this way serve to explain the general absence of clastic material in the chert." On page 466 he says: "At different more or less pro-

[1] Bull. Geol. Soc. Am., Vol. III, p. 394.

longed periods during the accumulation of the series the bottom of the sea sank sufficiently rapidly to be out of the reach of littoral sediments."

According to the observations of the author the body of the limestone is as free from detrital matter as the jasper, and except for the thickly scattered calcareous remains shows no more traces of organic origin than portions of the jasper.

I cannot see that the origin through springs has anything whatever to support it. It is undoubtedly true that a subsidence or change in ocean currents gave rise to conditions favorable to the accumulation of beds of jasper and limestone. It is, however, rather difficult to believe that this movement, which could not have been of a catastrophic kind, should have accorded exactly with the flow of hundreds, if not thousands, of springs over the sea bottom, which at one period were purely siliceous, and at another deposited nothing but pure carbonate of lime. If the currents were as strong as the author supposes, it does not seem possible that the radiolaria should have settled so thickly as we frequently find them, and besides the springs possibly being fresh would not form a congenial place for marine organisms.

A short discussion is given by the author to deposits which he terms silica-carbonate sinter,[1] the true nature of which seems not to be understood. He says: "Its occurrence in extensive sheets, roughly parallel with the bedding, suggests that it is a contemporaneous deposit, but it may possibly be a vein formation. Its occurrence in the Aucella sandstones elsewhere, and in the San Francisco sandstones of the peninsula is of interest as a possible factor in the correlation of these formations." This is a case in which wider familiarity with the Coast Ranges and their mineral deposits would have readily settled a very simple question. These deposits of sinter are almost always associated with quicksilver ores forming their gangue. The quicksilver deposits are known to date from post-Miocene times, and owing to their recent formation it is to be expected that

[1] Am. Geol., June 1895.

they would occur at all geological horizons. Consequently the correlation of the Knoxville with the Franciscan series (Golden Gate series) by this means is out of the question. The sinter is very similar all through the Coast Ranges, and any deposit is a possible source of quicksilver.

Professor Lawson's discussion of the structure of the older uncrystalline rocks shows, although he minimizes the importance of the disturbances which they have undergone, that it is often difficult of elucidation. The earlier observers have all remarked upon the difficulties connected with a study of the so-called metamorphic rocks, and we cannot admit that all of their work is "superficial." While the author is probably right in asserting that the larger structural features are comparatively simple in the area under discussion, yet it seems to me that there is good reason for believing that the structure is very complex in detail. This is shown by the fracturing and folding of the jasper bands; the frequent occurrence of crushed shale, and thin-bedded sandstone in a ruptured condition; and the presence of innumerable cracks and shear planes in the more massively bedded sandstones. Local areas occur, it is true, totally free from the effects of strain, but they do not dominate. A comparison of the sandstone at Point San Pedro with that north of the valley of the same name affords a good illustration. Both in the cliffs and on top of the point the sandstone weathers out in large blocks in a manner closely simulating the sandstone in the Cretaceous and older Tertiary in the Coast Ranges. On the contrary the similar sandstone in the hills north of San Pedro Valley breaks up in angular fragments; being permeated in many places with veins of quartz and calcite, or linear seamlike cavities. This fracturing and veining is also almost everywhere to be noted in the jaspers and limestone. Professor Lawson considers that because the number of parting planes decreases downward from the surface they are due in great part to atmospheric agencies alone, although he recognizes some shear planes. I believe, however, that we have good reason for holding that a large part of these planes which separate the sandstone into

angular fragments are not due to atmospheric agencies alone, but that they are capable of being produced in thick-bedded rocks as well as in thin-bedded which have not been greatly sheared, but subjected to a rending strain. Such rocks under proper conditions can become recemented and apparently as massive as before. The cracks may thus become completely closed, or left slightly open and filled with calcite or quartz. Subjected to atmospheric agencies lines of weakness are soon developed, and the rock crumbles in angular pieces. A similar rock which has not undergone this breaking strain will weather either in large rounded knobs, or, if it be soft and argillaceous, break up into fragments more or less conchoidal.

Professor Lawson remarks further upon this subject as follows: "The superficial study of this phenomenon has led to grossly exaggerated views as to the amount of disturbance (shattering) to which the Coast Ranges have been subjected. The sharply marked alternation of wet and dry seasons, combined with the treeless character of many of the ranges, is peculiarly favorable t) this disintegration."

It is only necessary to compare the most of the sandstones of the Golden Gate series with those of the Cretaceous or Tertiary to see the vast difference in the manner of weathering. As a result of my experience through nearly the whole of the Coast Ranges, I feel satisfied that peculiarities of climate have not been the cause of the phenomenon under discussion. The abundance of cracks and shear planes in the older rocks is one of the chief reasons why it is so difficult to obtain good building stone from them, although the sandstones are characteristically thick bedded.

I think that Professor Lawson has underestimated the amount of disturbance to which the Golden Gate series has been subjected. It is not necessary for the folds to be involved or intricate, although they certainly are in many places, for it to have undergone a large amount of strain, fracturing, and shearing.

Professor Lawson professes to be entirely ignorant of the

"orogenic movements which effected the deformation and faulting of the Franciscan series" as well as of the relative sequence of these disturbances and the peridotitic intrusions. This is, of course, excusable in one studying only a narrow field where a part of the record is wanting, but too much is at present known of the wider field of Coast Range geology for one to plead ignorance on these questions.

The initial disturbance of the Golden Gate series, with the exception of that produced by the contemporaneous intrusions and flows, dates from the post-Jurassic upheaval. Leaving out of account the serpentine, there is evidence that many of the eruptive bodies associated with this series were subsequently formed. The intrusive nature of the diabase at Hunters point is recognized by Professor Lawson, and he is certainly correct. To the presence of these eruptives, I believe, is to be attributed the extreme disturbance in local areas.

As to the age of the Franciscan series[1] of Professor Lawson nothing more definite is advanced than the evidence of a few imperfect fossils, which cannot be determined specifically and in many cases not even generically. These fossils are supposed to favor the old view of a Cretaceous age. He says: "Evidence, such as it is, is confirmatory of the opinions of Whitney and Becker. . . . . The series as a whole is very probably older than the Knoxville Aucella horizon of California. The writer has no doubt upon this point."

Professor Lawson gives no reason for assuming the series to be older than the Knoxville, and since Mr. Stanton's[2] work places the Knoxville *Aucella* horizon at the base of the Cretaceous, it is difficult to understand how these pre-Knoxville rocks can be included in the Cretaceous.

[1] This designation (American Geologist, June 1895) embraces the same aggregate of strata to which I have given the name Golden Gate series (JOURNAL OF GEOLOGY, May–June 1895) from its characteristic exposures at the entrance to San Francisco Bay. As to which name shall finally be accepted I am, for my part, willing to rest the case, although another claim might with great justice be added, on the truth or falsity of my published statements as to its age and stratigraphic position.

[2] Bull., No. 133, U. S. Geol. Sur.

Again Professor Lawson argues for the post-Jurassic age of the granites in the Coast Ranges, and in order to account for the fact that the pre-Knoxville rocks rest on the granite with a basal conglomerate, he is obliged to assume that they are a part of the Cretaceous. The correctness of one view argues much for the other; if one falls both must be considered invalid.

Again he says: "Fairbanks has combated the views of Whitney and Becker, and has pronounced the series to be of pre-Cretaceous age. He has not yet, in the writer's opinion, established the correctness of his contention." I wish to call especial attention to this statement because of the fact that Professor Lawson has not advanced one particle of evidence in refutation of my published statements as to the existence of a nonconformity between the Knoxville and these lower beds. It is hardly probable that we shall soon find any fossils in good enough condition to be decisive concerning the question at issue, and the main dependence must be placed upon stratigraphy. The fact that these rocks (Golden Gate series) lie unconformably beneath the Knoxville is shown by the most strongly marked contrast, structurally as well as lithologically, in addition to the stratigraphic break which I have described in previous publications.

In closing his description of the stratigraphy and structure of the Franciscan series, Professor Lawson says in regard to the disturbance of the strata: "The most of it seems to have long antedated the uptilting of the Montara fault block, so that the latter differs from most tilted blocks with which we are familiar. These are commonly tilted blocks of strata previously undisturbed, and the tilting is recognized by the attitude of the strata and the presence of fault scarps. In the present case, however, the region has been moderately folded and profoundly faulted with local sharp plication." As far as I am conversant with the geology of California and adjacent parts of Nevada, the fault blocks are commonly not tilted blocks of previously undisturbed strata, but exactly the opposite; that is, the strata have been more or less highly folded and tilted previous to the faulting,

much of which now recognizable, is of quite recent date, geologically speaking.

In the last sentence quoted Professor Lawson seems to recognize quite fully the complicated structure of the Franciscan series.

The serpentine in the area covered by Professor Lawson's report is divided into three linear tracts. One extends across the city of San Francisco from Fort Point to Hunters Point, a distance of ten miles, with a width of one and one-half miles. Another large tract extends from San Andreas Lake to San Mateo Creek, having a length of eleven and one-half miles and a maximum width of one mile.

According to the description these bodies occur as laccolites, laccolitic sills, or dikes. From a careful perusal of the report, and an examination of several of the more important areas covered by these eruptives, I cannot understand the reason for applying to them the designation laccolite. This term was originally given by Gilbert to an eruptive mass which in the course of being forced upward, instead of reaching the surface, finally spread out between the strata, forming a thick lens, and arching them over it in dome form. Professor Lawson is certainly right in the statement that the main bodies are not true dikes, but it seems to me that the term sheet or sill which he uses quite frequently is the really proper one for these eruptives. If the term laccolite has any exact meaning it is certainly not synonymous with sheet or sill.

I have carefully examined the so-called laccolites of Hunters Point, Potrero and Fort Point and can find no evidence that these eruptive masses were ever covered by an arched roof. In my opinion the field relations indicate that they cooled as sheets or dikes. They have no appearance of being lens shaped, and if it is true, as Professor Lawson supposes, that they are all connected through the distance of ten miles, we have then a long sheet inclined at an angle of 35° to 40°; and though in a general way intruded along the bedding planes of the sandstone and shale, yet, owing to the marked irregularities of the surface of the erup-

tive sheet, the inclosing strata have been much disturbed and show locally a marked variation in strike and dip.

The remnant of a supposed laccolite roof on Hunters Point I would interpret to be a body of jasper and sandstone or shale inclosed in the serpentine, as a portion of it occupies a sag between higher serpentine ridges, and apparently extends down into a small ravine.

It is fully as difficult to believe that the Fort Point occurrence is a laccolite. The stratum of shale, sandstone, and occasional bodies of jasper, which appears in the cliffs and along the beach south of the fort, dips into the cliffs at an average angle of not less than 30°, and it seems to me that it can be nothing else than an inclusion between two sheets of serpentine.

The exposures at the Potrero are good and bear out the opinion which I have already expressed.

That the two dominant ridges, Montara Mountain and San Bruno Mountains, are fault blocks of such importance, or their diastrophic history so clear as Professor Lawson outlines it, does not seem evident to me. The San Bruno Mountains are said to be the older block, but the southern slope facing the supposed fault is remarkably bold and steep, not showing an advanced stage of degradation, and in addition the northern slope is almost as abrupt. The supposed fault of 7000 feet appears almost incredible, and it does not seem at all necessary to postulate it in order to account for the position of the Merced series. That this series should once have existed over the whole of the northern end of the San Francisco peninsula appears very problematical at least. That a series over a mile in thickness, and of so late an age, should have been so completely removed as not to leave a trace north of the supposed fault it is not easy to believe.

I cannot find any evidence either of the supposed great fault on the southern slope of Montara Mountain. The topography as a whole does not support the idea, and that the ridge is a simple tilted block is not supported by the evidence which Professor Lawson adduces of important faults on the northern slope. Professor Lawson's suggestion in another place that Montara Moun-

tain has been forced up through the strata in the manner of a telescopic thrust appears to me to have an important element of truth in it. This thrust probably resulting from lateral compression produced the elevation of the granite, partly, at least, through shearing.

It would appear that the evidence in favor of the successive appearance of the "two dominant fault blocks" is exceedingly slight. While the San Bruno Mountains are considered to have been above the sea level and the Pliocene undergoing erosion, there is believed to have been no such subaërial period for Montara Mountain until the final uplift resulting in the terrace deposits. This is opposed to the observation of Mr. Ashley[1] as well as to my own views concerning the general post-Pliocene elevation of the coast. By this elevation I do not mean that to which the terraces are due, but an earlier one resulting from the same influences which deformed the Pliocene sediments the whole length of the California coast.

In regard to the age of the granite Professor Lawson[2] says: "The simplest and most natural hypothesis that suggests itself is that the granite corresponds in age with that of the Sierra Nevada, and this hypothesis has not yet been exhausted of its strong probability of truth. The granites of the Sierra Nevada, in so far as their age is known, are clearly post-Jurassic. Granites of about this age are extensively developed along the west coast of North America from Alaska southward. The granites of the southern and northern Coast Ranges seem to be geologically continuous with those of the Sierra Nevada. The fact that the Sierra are separated from the Coast Ranges by the valley of California is immaterial to the discussion, since the latter is clearly a delta-filled geosyncline of late Tertiary or post-Tertiary origin. There is therefore a strong presumption in favor of the view that the granites of the Coast Ranges and those of the Sierra Nevada are of common origin and common history. This presumption must be steadily kept in view till it is negatived by positive evidence."

[1] Neocene Stratigraphy of the Santa Cruz Mountains, p. 334.

[2] Am. Geol., June 1895.

It does not seem to me that there is any validity whatever in the above reasoning, and in the light of the true position of the Golden Gate series there is a strong presumption in favor of an opinion exactly opposite to that just quoted. In fact the presumption is so strong that it amounts almost to a certainty that the granite in the Coast Ranges is older than the main body of the granite in the Sierras. What we at present know of the position of the Golden Gate series points to the fact that its first upheaval was contemporaneous with the last great upheaval recorded in the rocks of both the Sierras and Klamath Mountains. The Mariposa beds involved in this upheaval in the Sierras are held to be Upper Jurassic. The Knoxville beds deposited after this upheaval are believed on the best authority to be Lower Cretaceous; and if we make the granite in the Coast Ranges the same age as that in the Sierras, we must crowd into the break between the Mariposa beds and the Knoxville, a series of beds thousands of feet in thickness and separated from the Knoxville by a break as profound as that between the Knoxville and the Jurassic.

There is every reason for assuming that the granitic rocks of California are not all of the same age. Granitic bowlders occur in the Mariposa beds south of Colfax, a fact pointing to a pre-Jurassic granite body in that region.

The youngest fossiliferous rocks associated with granite in southern California are probably of Carboniferous age, and while the extension of the crystalline basement rocks of that region northwestward into the Coast Ranges is not likely to be much younger than the Carboniferous, for all that we know at present it may be much older.

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